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THE JOURNAL OF PHILOSOPHY
PSYCHOLOGY AND SCIENTIFIC METHODS

RITTER'S ORGANISMAL CONCEPTION OF LIFE

RITTER'S *Unity of the Organism*, the first of the three titles listed below,[1] is a work which calls, I think, for special attention from students of philosophy. The work embodies, on the one hand, a notable contribution to the philosophy of the organism, from the point of view of a fairly extensive acquaintance with the literature of philosophy; and at the same time an acutely critical review of the facts and tendencies—with attention centered upon the facts—developed in the past generation of biology. The two smaller volumes throw very interesting side-lights upon Ritter's philosophy. They are full of suggestions for the philosopher, but they strike one as desultory and unfinished. *The Unity of the Organism* bears the marks of years of thoughtful preparation. Ritter is everywhere agreeable reading. His style is frankly and easily personal, free from all scientific pedantry, and his criticisms, severe and outspoken, are always good-natured.

The organismal conception of life is developed by contrast with the various "elemental" theories so prominent in the biological thought of twenty years past. Under elemental theories Ritter includes not only those which regard the organism as a mechanical aggregate of elementary units, *e. g.*, as a mosaic of cells, but any theory which, like the germ-plasm theory, or the chromatin theory of inheritance, treats one "element" of the organism as more real or more determining than any other. To all such Ritter opposes "the unity of the organism," the determination of everything by "the organism as a whole;" which is not a name, but a thing, not a group, but an individual; at least as real and as determining in itself as any distinguishable "element."

[1] *The Unity of the Organism or the Organismal Conception of Life.* WILLIAM EMERSON RITTER, Director of the Scripps Institution for Biological Research of the University of California, La Jolla, California. Boston: Richard G. Badger. 1919. 2 vols. Pp. xxv + 806.

The Probable Infinity of Nature and Life. Three Essays. WILLIAM EMERSON RITTER. Boston: Richard G. Badger. 1918. Pp. 164.

The Higher Usefulness of Science and Other Essays. WILLIAM EMERSON RITTER. Boston: Richard G. Badger. 1918. Pp. 146.

Thus the unity of the organism comes to mean, even more distinctly, the uniqueness of the organism, and, in the last analysis, of the individual organism. Ritter's idea, for which he claims the support of histology and biochemistry, is that every tissue and every chemical reaction, whatever general features it may have, is also characteristic, not only of the species in question, but of the individual. There is no protoplasm, he maintains, but only *protoplasms*. And this means, carried further, that the unity of the organism is the unity of the *living* organism. Nothing is more insistently emphasized throughout the two volumes than the difference—a difference that must be conceived to extend to every detail of composition and structure—between the living and the dead animal. That the zoologist of to-day, a laboratory zoologist, is mainly a student of dead animals, is food, not for humor merely, but for thought. The Cartesian problem of mind and body seems to be based upon the dead body; as commonly conceived, it is a problem of mind and corpse. And biochemistry—from which, indeed, Ritter derives his own conception of the organism as a chemical laboratory and as a chemical element—is also mainly an analysis of dead animals, or at best of the dead products of the living. "The naturalist accepts not only without hesitation but with eagerness and gratitude the chemist's report on what he *is able to get out of the organism,*" but, "knowing as he does something of the methods by which the chemist gets at the chemical substances of organisms," he can not suppose that the chemist's reports "come near setting forth what the organism *actually is.*"

Ritter speaks here as "the naturalist." This term embodies comprehensively his personal philosophy and his conception of the scientific attitude. Speaking always as a scientist, he holds that the scientific attitude is represented more truly by Darwin than, say, by Loeb; by contact with nature in the field than by mere laboratory analysis; and, incidentally, by breadth of view than by narrow specialism. Elementalism, issuing in a mechanical and materialistic theory of life, is the consequence of supposing that the products of the laboratory are exclusively real. The result is not science, but "metaphysics."[2] Science in the true sense is based upon a comprehensive observation of fact. And therefore—the laboratory preju-

[2] Ritter supposes that his own method is free from metaphysics. Yet as a programme for description he postulates the distinction between attributes of individuation and attributes of relation (*The Probable Infinity of Nature and Life*, p. 72), and all of his thinking seems to imply that the world is made up of things and their relations and not merely of groups of elementary attributes, or "phenomena"—clearly a metaphysical proposition. To my mind it is about as possible to eliminate metaphysics from thought as to eliminate respiration from life.

dice to the contrary—nothing is more worthily or more importantly scientific than the work of description and classification.

One is prepared, then, to learn that, as a Darwinian in science, Ritter is an Aristotelian in philosophy. He makes the suggestion—very fruitful, I think, for an interpretation of the Aristotelian metaphysics—that Aristotle was primarily a zoologist; and I should say that Ritter's whole work is, both in the aspects emphasized and in the difficulties neglected, a characteristic expression of the Aristotelian point of view in science. The keynote is a radical empiricism, recalling in its freedom from logical and scientific convention that of William James, which will decline, if possible, to treat any aspect of experience as less real than any other. It is radical empiricism that he opposes to elementalism. For he is equally opposed to vitalism. In Ritter's view Driesch's entelechy (in spite of the derivation from Aristotle) is no less an abstraction than Weissmann's germ-plasm. It represents an attempt to explain the phenomena of life by something less than the organism as a whole.

It is not easy to convey in summary the effect of an argument involving such a mass of detail and so much shrewd suggestion. Among the elemental theories refuted are: the Weissmann theory of an independent and all-determining germ-plasm; the theory which makes the organism merely an aggregate of chemical substances and processes; the theory of a universal protoplasm; the cell-theory, which explains the organism as a mosaic of "simple" cells; the chromatin theory, in which the chromatin of the chromosomes is treated as the sole "hereditary substance;" the theory that internal secretions are "formative stuffs;" and the Loeb theory which conceives the nervous system as an aggregate of originally independent, chemical "tropisms." Ritter is not slow to recognize the advances in biological knowledge which have been stimulated by these elemental hypotheses; his point is that none of the elements can be regarded as the "key" to the organism or in any exclusive sense as a carrier of heredity. It is the "nothing but" aspect of the elemental theories which he mainly contests. And the very idea of a "carrier of heredity" he is disposed to condemn as a superstition akin to phlogiston. Granting that a starfish produces an egg and that the egg gives rise to another starfish, does any biologist think that only a sufficiently powerful microscope is needed to enable him to see something in the egg "carrying" all of the innumerable characters of the adult starfish? What he might expect to see would be certain structural features peculiar to the starfish at the egg-stage of the individual's life; which would then disappear and be supplemented by other features peculiar to the embryonal stage—and so on (I., 224). In brief, re-

garded as an observed fact, heredity—which, by the way, applies not to adult characters solely but equally to those of every stage—is a process of transformation, not of transmission.

To the Weissmann theory which, on behalf of the continuity of germ-plasm from generation to generation, seems to call for the independence of the germ-layers and the origination of sex-cells in the outer layer, he opposes, among other observations, the appearance of sex-cells in the endoderm of hydroids; and Weissmann's attempt to account for this by "migration" he characterizes as a curious "example of the effect on the observing powers of the germ-plasm type of speculation." To the chemical theory, which makes the organism a chemical *product,* he replies by pointing out that each organism is a chemical *laboratory,* manufacturing its own specific product—as shown by differences of odor in plants and animals, by Reichert and Brown's results on hemoglobin, by the precipitin reaction as between bloods of different animals, by the "comparative chemistry" of the sperm of fishes, of milk, of digestive enzymes, *etc.* His extensive and (to the outsider, at least) very instructive examination of the cell theory is devoted to showing, mainly through a discussion of observational evidence, that the cell is peculiar to the organism and always the product of an organism, never a prior and independent unit; that the unicellular organism is still an organism (its "simplicity" being an exaggeration on behalf of a supposed pedagogical convenience), and the egg an organism in the unicellular stage; and that the attempt to treat protista as cells results only in showing, if anything, that beings much smaller and considerably simpler than cells existed long before cells.

The most extended treatment is accorded to the chromatin theory; which supplants the cell theory. Adopting Castle's definition of heredity, which defines heredity simply as "organic resemblance based upon descent," Ritter does not deny that "*to some extent* resemblance between ancestors and progeny is *in some way* connected with chromosomes." Not many of the major theories of biology are more securely established than the chromosome theory. His contention, however, is—based upon a lengthy examination of the evidence from protozoans, from the metazoan germ-cells, and from somatic histogenesis in multicellular organisms—that the cytoplasm, as well as the nucleus as a whole, is no less responsible; and that the inheritance materials of germ-cells are initiators rather than determiners of heredity.

The same mode of argument is applied to the theory of internal secretions. For example, as bearing upon the metamorphosis of the tadpole into the frog, "the truth appears to be that thyroid sub-

stance is organ-forming in much the same sense that water is organ-forming for the leaves, flowers, and fruit of the squash vine. . . . That is, an under-supply of water has an effect upon immature plants similar to that of an over-supply of thyroid substances upon immature frogs, namely, that of retarding growth and hastening metamorphosis. . . . Thyroid substance is organ-forming only through being organ-transforming" (II., 145). Again, his point is, with Sherrington against Loeb, that the simple reflex arc is an abstraction. "No one should be beguiled into the notion that the readily observed facts of ontogeny of the nervous system, the various processes, dendrites, and axones, do actually grow out on nerve cells and bring cells into connection with one another and with receptor and effector cells, and that a functional coordination is thus finally reached [which] does not exist in any way or degree in the early stages" (II., 169). "Every specific act of every part of the nervous system is primarily in the interest of some other part and function of the organism than itself" (II., 184). Even the antagonisms between reflexes (which, by the way, never lead to the disruption of the organism) are "constitutive of the normal organism. Even the most pronounced of them are yet in the interest of the organism as a whole" (II., 324). In passing from neural to psychical integration we find him—precisely in line with his general position—with the apperceptional as against the associational, or elemental school, standing for the rôle of mental activity in the development of thought.

Ritter's constructive argument, the main lines of which have been already suggested, consists chiefly of evidence for integration, *i. e.*, the influence of the organism as a whole in the production of each part, distinguished under the heads of growth integration, chemico-functional, neural, and psychical integration. Very interesting is the chapter on growth integration, in which he points to the existence in all growth of graded series of parts or processes (illustrated most simply in the tapering of a leaf or of the skeleton of a python) and calls to his aid Child's demonstration of "axial metabolic gradients," *i. e.*, gradients in rate of cell division, size of cells, rate of growth, and rate and sequence of differentiation, which are definitely related to the axes of the individual or its parts.

But the most striking feature of the organismal theory is the organismal conception (admittedly hypothetical) of consciousness. Among the several elemental theories, that with which Ritter seems chiefly concerned to come to terms is the chemical theory. That every organic process is a chemical process is treated as indisputable. But if so, how are we to attribute a real unity, implying individual-

ity and creativeness, to the organism as a whole? A reply already given is that the organism is itself a chemical laboratory; not a product, but a source of chemical change. Along the same line, the organismal theory of consciousness holds that each living organism has the value, chemically, of an elementary substance. To make the meaning of this clear, Ritter explains that fundamentally, from its beginnings in alchemy, chemistry is a study, not of the composition of things, but of their transformation. Hydrogen unites with oxygen so as to produce new attributes not prefigured in either element. Hydrogen is thus creative. And thus also the organism. From the chemical standpoint, the fundamental aspect of all life, as conceived by Ritter, is the transformation effected by the organism through contact with the gases of the air, as typified by respiration. In this process are created all of those attributes, physical or mental, which we call "life."

Such a creative transformation, for example, is knowledge. Ritter quotes the question raised by Hume: how can I infer the "secret power" of nourishment from the sensible attributes of bread? Or, from one instance of nutrition how can I infer another? What is the "medium" of inference? Ritter replies that the medium of inference, and the source of the "secret power," is the individual organism reacting in an enormous complexity of ways—mostly revealed by natural science since Hume—with the respiratory substance it takes in (II., 301). And thus he accords a certain justification to the Cartesian theory of innate ideas, in the sense, however, of hereditary potentialities. This is not to say that knowledge is merely subjective—no more, perhaps, than water is subjective to hydrogen. Knowledge is a process of transformation involving both subject and object, both knower and known.

Ritter calls this a conception of "consciousness." What it undertakes to make intelligible is the possibility of individuality and creativeness in something chemically constituted. It seems to me, however, that the question confronting a theory of *consciousness* is rather this: When hydrogen effects, with oxygen, a transformation into water, we can ask how it *looks* to the observing chemist; we can not (in the view of science and most common sense) ask how it *feels* from the point of view of hydrogen. Of any human activity we can ask both questions. How are we to explain the difference? Ritter's reply would be, I think, a refusal to assent to the current separation of the "inner" and "outer" aspects of life. At least it seems that, throughout the organic world, an inner aspect exists for every outer. For "the psychical aspect" is not restricted to the nervous system: it is everywhere "latent," at least, in "the breath of life," that is,

wherever there is a chemical reaction of an organism with the gaseous constituents of the air (II., 303). But the whole organismal conception seems to imply an essential continuity of organic and inorganic. All creativeness, Ritter tells us, chemical creativeness with the rest, is known "through being in our own deepest natures creative" (II., 295). And he more than once derides the scientist who thinks that the epithet of "anthropomorphism" is an answer to an argument. It would seem, then, that the organismal conception points in the direction of panpsychism. At any rate, as against the idea that the higher stages of evolution contain "nothing but" what was found in the lower, Ritter holds that the higher are a fresh revelation of the nature of the lower.

And thus when we ask how the behavior of the organism as an element is to be related to the elements found in it by chemical analysis —how the "chemistry" of social and spiritual life is related to the chemistry of the laboratory—the answer is that "the psychic activities of men, particularly the imagination and the emotions, reveal the fact that carbon, oxygen, nitrogen, and the others, are infinite as to their attributes of relation, exactly as water reveals a few attributes of relation of oxygen and hydrogen, and as table salt reveals a few attributes of relation of sodium and chlorine." At least it is true "that we have experimental evidence of their possessing a vast amount and variety of energy, and no ground whatever excepting the limitations of our momentary laboratory information about the substances, that the number and measure of their energies is limited."[3] If, in other words, the organic phenomena are chemical, and if also they are really organic and spiritual activities of real individuals, then it must be that the chemical properties of substances are only very partially revealed in the chemical laboratory.

Such is the biological outcome of a "naturalistic" point of view, *i. e.*, of a thoroughgoing empiricism, which accepts as real whatever is found in observation and refuses to be bound by predetermined criteria of reality. Ritter's book suggests many questions, of which I will point to only one. Ritter calls himself, very truly, I think, a "naturalist," but he is no less insistent in claiming to speak in the name of science. With all his strictures upon current scientific theories, he writes as one who believes that the only truth is the truth of science. Now, I find it rather difficult to identify "the scientific attitude" of to-day with an exclusive regard for the results of observation. Science, like the church, may be militant or triumphant. Science militant (more in evidence a generation ago) is quite pious in pleading only for "the modern spirit of free in-

[3] *The Probable Infinity of Nature and Life*, p. 124.

quiry." Science triumphant stands firmly for the "fundamental" truths, or laws (such as gravitation and the conservation of energy), established by the fathers. "Elementalism" is a case of science triumphant; it represents the claim of suzerainty on the part of the older sciences of physics and chemistry over the newer science of biology. Ritter's naturalistic logic is obviously militant. Yet he is none the less loyal to the law of conservation of energy, as a law established, seemingly, once for all.[4]

This raises the question that I have in mind: how is the organismal theory of life to be reconciled with the law of conservation of energy? It strikes me that this is the largest question that the organismal theory will have to meet, and I wonder therefore that the question is nowhere broached in *The Unity of the Organism*. In this question we have the biological version of the eternal problem of continuity and change. The organismal theory stands for the reality of growth and change—for "creativeness." The conservation-law evidently knows nothing of creativeness. All that it finds in nature is a *redistribution* of energies, elements, or what not, on the basis of a quantitative equality of antecedent and consequent. And positively it seems to reject creativeness. For any influence at work directing the redistribution towards an organic end would seem to imply some additional "energy" at work not subject to the conservation-law; and, therefore, not to be tolerated. Ritter speaks at times of the organism as if it were just such an additional agency; for example, when he is compelled to the "assumption that the organism 'taps' or unlocks energy attributes of the elements."[5] This looks very much like the repudiated Drieschian entelechy. But the question is just this: how will the unity of the organism dispense with an entelechy, or something of the sort, and yet avoid being wiped out by the conservation of energy?

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TRUTH, VALUE AND BIOLOGY

I WAS delighted to learn from Professor Wells's article on "The Biological Foundations of Belief"[1] that he has "the habit of regarding all human questions from the biological point of view," and so has a fundamental point of agreement with me. For if he is right in thinking so, we may be able to cooperate further in the discussion of the important question of the biological control of human beliefs.

[4] *The Probable Infinity of Nature and Life*, p. 77.

[5] *The Probable Infinity of Nature and Life*, p. 92.

[1] In this JOURNAL, XVI., p. 259.

Long experience, however, of the possibilities of intersubjective misunderstanding among philosophers admonishes me to caution; I should like first to make sure how far this agreement extends, and in particular, whether we can agree upon the meaning of the chief terms involved. For otherwise no profitable discussion is likely to result.

Unfortunately it is just this point which, in spite of Professor Wells's assurances, still appears to me to be in grave doubt. I may illustrate my difficulty from Professor Wells's accounts (1) of the "Pragmatic Fallacy," (2) of the meaning of truth, and (3) of the relation between logic and psychology.

1. In his first paper[2] Professor Wells gave the name of *Pragmatic Fallacy* to what he considered a "confusion of truth and value," and appeared to me to illustrate a tendency, still unfortunately common among philosophers, to regard a pragmatist as an imbecile who is incapable of understanding the simplest usages of popular language, and who can therefore be triumphantly confuted by showing that, until he commenced to undermine immemorial usage, every one had always 'understood the words to mean' something quite different from the interpretations pragmatism now sought to put upon them. Accordingly I had to point out that what he condemned was an unconscious fusion or psychic coalescence of truth and value, which is natural to the human mind, and which the pragmatists had been the first to expose. It seemed a little hard on them thereupon to name this tendency after them, and a little hasty to condemn it utterly before examining whether there might not be good ground for it. To this complaint of mine no satisfaction has been conceded; Professor Wells still calls the tendency in question "the Pragmatic Fallacy," in spite of having a reasoned pragmatic repudiation of it before his eyes. Indeed he appears fully to justify my complaint by explaining that when he defined the Pragmatic Fallacy as a confusion of truth and value, he was *not* using 'truth' in the same sense as the pragmatists. So these unfortunates are not only required to accept, without investigation or criticism, a definition which analyzes the situation as a 'confusion of truth and value,' but also to use 'truth' in a way they have declared to be unmeaning, and finally to ascribe *to themselves* the absurdities that result from this procedure! These are terms which only a complete victor could dictate.

2. As regards the meaning of 'truth,' Professor Wells's first paper gave no inkling of what he meant by it: the second does indeed explain it, with admirable clearness, but still in a way sugges-

[2] This JOURNAL, XIV., p. 653.

tive of the unfortunate tendency referred to above. For I can hardly believe that Professor Wells supposes me to be ignorant that "common sense and science assert that 'truth is so,' whether or not it is known by any human mind,"[3] that "in popular or common sense usage 'truth' is thought to mean simply what is so," and that "in both popular and scientific usage the truth is taken to be entirely independent of what any one may *like* to believe, or of what any one may be led to believe for 'subjective' reasons."[4] Nor can I easily believe that *he* was ignorant that it is precisely this popular usage and the false conception of truth on which it rests, which pragmatism has set itself to challenge, and amend. But, if so, what is the good of appealing to pre-pragmatic usage in pragmatic controversy? The correctness and value of "established usage" is precisely the point at issue, and to presuppose a pre-pragmatic sense of 'truth' is merely to beg the question. Pragmatism can only be refuted, if it can be shown to disregard a sense of truth it has itself accepted.

Professor Wells, therefore, should expect to be told that science, though it starts from common-sense, is progressive, and so is capable of revising the notions it takes over; nay that even common-sense is teachable, though it is slow to learn. So if it can be made clear to science that the phraseology about 'truth,' which it has very naturally inherited from common speech, is untenable and was based on ignorance of biological, psychological, and sociological investigations, which go to show that every 'truth' in every science is necessarily conditioned by influences deriving from these sciences, every science worthy of the name will gladly take account of this enlightenment. Nor is there any reason to suppose that science would scruple to admit that the depersonalization of truth, which is so convenient for some purposes, is strictly a methodological fiction, or would demur to obey the summons of pragmatism and disallow scientific investigations into the limits of its validity; the more so that even philosophers can occasionally be found to analyze and discount their personal bias; as in the noble example recently given in this JOURNAL by Mr. Bertrand Russell.[5]

Common-sense is more inert, but even that is not so pachydermatous as to be utterly insensible of the inconsistencies and contradictions in which it finds itself involved. Among these, of course, the instability of 'fact' and the constant transformations and trans-

[3] *L. c.*, p. 267.

[4] *L. c.*, p. 268.

[5] XVI., pp. 18–20. I should agree of course that "complete escape from personality" is impossible, but insist also that the "partial" escapes so pertinaciously advocated are one and all illusory.

valuations which the 'facts' undergo in the growth of knowledge, the impossibility of reaching any 'fact' that can seriously claim to be absolute or absolutely independent, and of sharply distinguishing between fact, interpretation, theory, hypothesis, and fiction, would be found to be relevant, and fatal to the simple-minded dogmatism of common-sense. At any rate it is perfectly futile to try to refute pragmatism by taking this dogmatism for granted as unquestionable.

3. Much the same might be said of the absolute distinction between psychology and logic. When Professor Wells declares that because "truth is depersonalized in popular and in scientific usage, truth is a logical and not a psychological matter,"[6] "in which only propositions, theories, hypotheses are involved, while the finding of these propositions, or the attempt to find them and to verify them, is a wholly psychological matter, of which truth and falsity may not properly be predicated,"[7] he must submit to be told that he is committing the most impossible, monstrous, and mischievous of false abstractions, against which all pragmatism is an unceasing protest, and that "a 'logic' which 'emancipates' itself from psychology will be a 'logic,' which, in repudiating its *raison d'être,* sinks to the level of a mere grammatical exercise,"[8] because in abstracting from personality it "abstracts also from the consideration of judgment as true-or-false." It may be said further that Professor Wells himself confirms this criticism by confessing that his 'logic' is capable of recognizing only propositions. A proposition, he tells us, "is not a response. It is, first of all, a group of words, which, as words, are marks on paper or sounds in the air. Words have a meaning however . . . a proposition, in the first place, is not a psychological subject-matter; and secondly, it is of propositions that truth and falsity are properly predicable."[9]

I believe that every one of these contentions is demonstrably false; and it is precisely this conception of logic that I have accused of wanton abstraction from meaning.[10] For meaning is not properly a matter of *words* at all, but of persons, and only persons can value a belief as true or false. Judgments also are the *acts* of persons, whereas Professor Wells's 'logic' knows nothing of judgments and is restricted to the propositions, *i. e., forms of words,* in which their meaning was conveyed. Professor Wells recognizes of course that his demarcation of 'logic' departs from common usage—at least as far, I should say, as does the pragmatic sense of 'truth'; but he re-

[6] *L. c.,* p. 268.
[7] *L. c.,* p. 270.
[8] H. V. Knox's *Philosophy of William James,* p. 83.
[9] *L. c.,* p. 261.
[10] *Formal Logic,* Ch. XXIV.

luctantly allows 'usage' to sanction our speaking of "true and false beliefs or judgments."[11]

To me, on the other hand, these are the really important things, which no 'logic' may sacrifice to juggling with 'propositions,' under penalty of extinction. Nor can I understand how Professor Wells can claim to have assimilated the biological point of view, unless he is willing to believe that "beliefs which a man can not *live with* he has no option but to discard; beliefs he can not *live without* he must find reasons to adopt. These too are corollaries from Darwinism, which philosophic theories must assimilate, if they themselves are to live."[12] All this seems to me to hold, not merely of religious beliefs but of views on logic and metaphysics as well, and the only way of continuing to profess unworkable beliefs would seem to be to hold them with a mental reservation that they must on no account be acted on.

4. These differences in the meaning we severally attach to the terms involved will not render it easy for Professor Wells and me to mean the same thing by the 'biological foundations of beliefs,' even where we use the same phrases. Still more of an obstacle, however, would appear to be created by his reticence about my conception of the relations of truth and value. I had in my article put forward a very definite proposal for treating 'truth' as a *species* of 'value,' and shown that every 'fact' and every 'truth' logically implied a claim to be *the best* of the alternatives that were within the cognizance of the science recognizing the 'truth' or 'fact' alleged. Nevertheless Professor Wells has not a word, whether of approval or of rejection, for this theory. Until he has made up his mind about it, he hardly seems to me to be in a position to approach the subtle and infinitely complicated problems which arise when we attempt to determine, more precisely and concretely, the actual extent of the biological influence on the beliefs that are in vogue.

For when we approach the actual complexities of human beliefs we speedily discover how inadequate to their analysis are such simple-minded maxims as 'facts are facts whether we know them or not,' or 'truth, like murder, will out,' or 'errors are mutable and fleeting, while truths are eternal and abide without change.' We find that in point of fact it is 'truth' that changes and 'errors' that persist unchanged from age to age, that facts which are unknown largely cease to operate as such, while illusions, superstitions, errors, and lies, which are believed to be facts, *ipso facto* become at least social facts, and grow a mass of evidence which bears them out, and

[11] P. 270, *cf.* p. 261.

[12] H. V. Knox, *op. cit.*, p. 93.

often seem as authentic as anything that is believed, while the more we pry into the credentials of our beliefs the more incredible it becomes than any 'truth' or any 'fact' should be absolute.

The student of beliefs, therefore, will not arrogate to himself the right of declaring that any belief has the supreme value ('validity') of an absolute truth about absolute fact; he will be modestly content with registering the varying values of beliefs, and will note their infinite gradations. Neither will he expect sweeping generalizations, such as that biological conditions must determine the survival of beliefs and that positive survival-value must entail acceptance as true and negative survival-value rejection as false, at once to clean out all the nooks and crannies of his subject.

He will reflect rather that beliefs admit of degrees and shades, of varieties and variations, and that few minds are so stable, tenacious or narrow, as to maintain any particular attitude of belief with full intensity of conviction for any considerable period, untempered by the corroding breath of doubt, and unmodified by the accretions of age and growing insight. He will find that half-beliefs and quarter-beliefs are common, and that some beliefs are intermittent and exhibit seasonal dimorphism, while beliefs that are relative to an occasion which evokes them are apt to pass away with the same. He will note further that beliefs may be more or less unconscious, and that men may be unaware of those that determine their actions. so that they may unwittingly (as well as consciously) give a false account of them. Some minds, he will find, are distracted by the open struggles of incompatible beliefs, all influencing their action and equally capable of determining it; while others seem to be built in logic-tight compartments, and suffer little or no inconvenience from sheltering inconsistent beliefs in different 'parts of the soul.'

When, in face of such a situation, he is called upon to consider the relations of belief to action, he will hardly be able to answer offhand. He will see that the biological test of survival-value may become hard to apply, because there are so many ways of more or less evading and defeating it.

Thus while it may remain undisputed as a general principle that action is the ultimate test of belief, it will not follow that this principle applies to everything that calls itself a belief. The questions will have to be raised whether what is professed is really believed, whether it is the professed belief or the real that determines action, or both in varying propositions, whether, when after a struggle a professed belief is *not* enacted, we may safely declare that it was not *believed.*

Again, how are beliefs to be dealt with which disclaim any connec-

tion with action, and profess to be purely 'theoretic'? It is clear that they need not lead to the consequences they should logically involve, on the assumption that their meaning and value are to be tested in the normal way, by action. And this for several reasons. In the first place action may now be guided by principles, which may either be depreciated as 'practical makeshifts' or be unavowed altogether, entirely different from those which are called 'theoretically true.' Hence the most futile or pernicious beliefs can now be held with impunity. For even though they might be fatal *if acted on*, yet since they are *not* acted on, their holders may persist and flourish; and their 'beliefs' with them. And yet, if they are not acted on at all, are they believed at all? Ordinarily, one would answer—'No, they are mere make believe and camouflage.' But here it is part of their case that, being *purely theoretic*, they *ought not* to be acted on. If therefore we abstain from pressing the subtler point, that to abstain from action on such a theory is really to act on it, we should appear to be baffled. Thus the testing of beliefs by action fails in the case of *complete intellectualism*. For in this case all connection between belief and action seems to be broken down altogether. Any belief may accompany any action, and no inference holds from action to belief or *vice versa*. This is so inconvenient a corollary that it is fortunate that complete intellectualism is extremely rare, and that for practical purposes it may safely be identified, less charitably, with *complete insincerity*. At any rate an adequate diagnostic for the discrimination of these two habits of mind would appear to be a great *desideratum* of intellectualist apologetics.

All these complications may inspire us with caution, and may receive further illustration, when we approach the difficult question which has been raised, that of the biological confutation of pessimism. It is indeed soon clear that the case of pessimism is not to be disposed of by a simple declaration that of course pessimism can not be true, because it is a belief that eliminates its holders. For how then could there continue to be pessimists, as there have been in all ages? There must be something, then, about the world that enables them to continue, even as a minute minority. What that something is is more difficult to determine. It may be that the pessimistic temper is correlated with other qualities, like caution, that are conducive to survival. It may be merely that pessimism survives, because it is not acted on. But it seems to be very unlike the 'purely theoretic' beliefs instanced above, and probably always affects action more or less. *Complete* pessimism is no doubt a difficult theory to act on; but so is perfect optimism: in fact the difficulties of both would appear to be essentially the same. But partial, or seasonal, pessi-

mism does *not* seem to be practically impossible. It may even be a better adaptation to the given circumstances than some forms of optimism. 'Bears,' as well as 'Bulls,' make fortunes on the Stock Exchange. It may be argued, therefore, that there must be something inherent in the constitution of things to which pessimism is a response, or even an adaptation, and that pessimism will be as permanent as that something.

But is not this to admit that pessimism is *pro tanto* 'true,' even on the vulgar or 'correspondence' view of truth? For it is to give it a basis in the nature of reality. No doubt as much or more might be claimed on behalf of optimism, if that too were taken in a partial or moderate sense. It would then follow that *both* pessimism and optimism were 'true' and rooted in reality, which naturally stimulated some minds to a pessimistic, and others to an optimistic, reaction.

'Fie upon the contradiction!' intellectualism would thereupon exclaim. But there would be *no* contradiction in the nature of the real. The real would really and objectively be such as to render *either* interpretation subjectively possible. It would be, if not neutral, at all events not such as to favor either party decisively. It would really be such as to stimulate one mind to a pessimistic, and another to an optimistic, verdict; nor could any amount of partisanship on our part induce it to alter its attitude. Moreover it would clearly be as 'objective' a fact as any other that the real appealed differently to different minds and was valued accordingly.

It may now be suggested that the case of pessimism does not stand alone. This sort of situation is in fact the rule. In every disputed question the parties to it will have a bias, and their bias will largely determine their answers. There will therefore be a psychological, subjective, or personal side to it, and it may often be the only side that matters. We all know that it is vain to recognize any other in dealing with the beliefs of lunatics and fanatics. In no disputed question can it be truthfully alleged that the nature of things imposes on us any particular answer. That is precisely why we feel free to believe as we like, or to let 'theoretic' considerations determine our beliefs. The real does *not* determine them for us with biological necessity. But our freedom in either case has an element of illusion in it. We are not *wholly* free, and the 'theoretic' considerations which *seem* to determine our beliefs are not ultimately theoretic. For the same reason in both cases, *viz.*, that the nature of things does exercise a certain control, and limits our freedom, though it does not destroy it altogether. This is just why it is so important to show that even in an extreme case, like that of pessimism, the biological determination of beliefs is not complete.

All of this, on a little reflection, will probably seem obvious. But is it not strange that, in face of it, any philosopher should seriously contend that the human and personal factor in beliefs must be ignored? For it appears to be the very factor to which alone we can look to transmute the ambiguous and indeterminate pressure of the uncomprehended real into definite judgments of affirmation or denial, according to the bent of the personality engaged; thus it is the sole factor which can engender truth and render reality comprehensible. It is possible, of course, to abstract from this factor; for it appears to be irrelevant for many scientific purposes: but nevertheless depersonalization is essentially a *fiction*. It is a fiction, moreover, which conceals from our view all the subtlest and most interesting influences of vital conditions upon beliefs, and renders impossible any coherent and intelligible accounts of the relations of truth and value.

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THE LOGIC OF PROBABLE PROPOSITIONS

FORMAL logic is an analysis of principles almost instinctive to common consciousness. Quite as instinctive are the principles of probable reasoning; yet philosophers have had little success in bringing them into the high-light of criticism, and less in uniting them with formal logic. The present paper is a suggestion toward attaining this analysis and this union.

A frequent misconception of the nature of probability will serve as starting point. A judgment is probable, say Laplace and his less illustrious follower Jevons, because he who judges believes, but is ignorant of the truth or falsity of his belief. Says Jevons:[1] "Chance exists not in nature and can not coexist with knowledge; it is merely an expression, as Laplace remarked, for our ignorance. . . . Probability belongs wholly to the mind." Probability, therefore, being intensity of belief, has the same status as anger or impatience or indignation; and one may well inquire how there can be any standard of probability. The only answer is: Since one belief, *quâ* belief, is stronger than another, the probability of the proposition believed is greater on account of the feeling of conviction which attaches to the belief, not at all on account of any character attaching to the proposition believed. But there must be some standard degree of intensity of belief as point of departure. The essential (and wholly conven-

[1] Jevons: *Principles of Science*, Ch. X., pp. 197–98. Also Laplace: *Théorie Analytique des Probabilities*, Introduction, p. ii.

tional) canon of this school is: Where we are ignorant a belief is as likely to be true as to be false; the probabilities of propositions of whose truth or falsity we know nothing are equal. However, no self-evidence goes with this canon. Seeing its lack of *a priori* validity and not wishing boldly to assume it, some theorists have called it a generalization from experience. Edgworth says in this connection:[2] "I submit, the assumption that any probability constant about which we know nothing in particular is as likely to have one value as another, is grounded on the rough but solid experience that such constants do as a matter of fact as often have one value as another." It is extremely doubtful that, being ignorant of the relative frequencies of events, we have learned from experience, *en bloc,* that our expectations are fulfilled about as frequently as not. Any investigation of statistics will yield thousands of cases in which the relative frequencies of events are not one to two. Had we begun in any of these cases with the equal distribution of ignorance, experience would have disappointed us by refuting our assumption. We might, quite as well, have distributed our ignorance in the ratio one to three, or one to six; and experience would, in a great many cases, have corroborated these distributions.

To place probability beyond the merely conventional, we must disentangle it from belief and attach it, not to beliefs, but to propositions believed.

Any belief is true, not by virtue of the fact that it is a belief, but by virtue of some sort of relation between the real world and that which is believed. When we believe or judge, there is always something which we believe or judge. Following current usage, we may call it a proposition. In addition there is always an object of which the proposition is judged true. This object lies somewhere in the realm of fact as a "locus of verification." To believe that something is probable is not to alter this situation. There are still belief, proposition, and fact. It is as absurd to hold that comparative intensities of belief determine the comparative probabilities of propositions as that irresistible conviction determines truth. Probability, like truth, gets determined by a relation between the proposition believed probable and the state of the facts. What is this relation?

Clearly, it is not the same relation to fact as that which a true or a false proposition has. The assertion, "He will probably die of influenza," is on different level of predication from the assertion, "He died of influenza." Fact (the time element being inessential) will ultimately reduce the probable assertion to truth or falsity, with no middle ground. As a probable proposition, however, it is neither

[2] Edgworth: "The Philosophy of Chance," *Mind,* Vol. 9, 1884, p. 223.

true nor false. Its probability comes from its reference to something other than the fact which will ultimately validate or invalidate it. Were fact its only referent, it must be either true or false. This other referent is *a class of propositions of which it is a member:* a class of propositions all of which make the same assertion, but which make this assertion for different instances or different situations. In the convenient phraseology of Mr. Bertrand Russell's logic, such a class of propositions may be defined as all those, and only those, propositions which result when a propositional function is given specific significant determinations of its variable.

It is evident that the members of a class of propositions of this nature, whether the class be finite or infinite, may be all true, all false, or a part may be true and a part false, this truth or falsity being fixed by a factual "locus of verification" and not by belief. Therefore, probability is frequency of truth within such a class of propositions. Fact determines the frequency for the class. And to qualify a proposition as probable is to ascribe to it a truth frequency which holds for its class. A probable proposition has as its first referent its class; as its second and indirect referent, through its class, fact.[3]

It will be well at once to make the distinction between finite and infinite classes. Obviously, a function could be true for all, for none, or for part of an infinity of values. Therefore, an infinite class of propositions would have a truth frequency, and any single proposition of the class would be probable; with this reservation, however: the probability would not be numerical since ratio has no meaning in infinite collections. On the other hand, in a finite class of propositions the truth frequency which we call probability may or may not, as we choose, be represented numerically. It will be the ratio of all the cases in which the assertion is true to all the cases in which it can be significantly made. The obvious cases of universal truth, or "certainty," and universal falsity, or "impossibility" are to be noted in passing.

The kind of bare, abstract probability attaching to every predication which can be made separately of a number of instances or situations is the simple element from which the richer form of probable inference can be constructed. The writer uses because of its clarity Mr. Russell's notion of a propositional function. Bare numerical probability is the proportional frequency with which any

[3] C. D. Broad: "On the Relation between Induction and Probability," *Mind*, N. S., Vol. 27, 1918, p. 389; views probability as an irreducible reference, a view to which the present writer can not agree. See also for a similar view, R. Demos: "A Discussion of Modal Propositions," *Mind*, same vol., p. 77.

propositional function will be determined as true by all values of its variable (the values being finite in number).

The measurement of any specific probability is difficult. Not all facts are given or are accessible to observation. So, it is impossible to know, when a class of propositions has been defined, how many of them will be true and how many false. Some simple cases are analogous to perfect induction and present no difficulty. Consider the well-worn example of the urn containing seven white and three black balls. If the balls be drawn and not replaced, the truth frequency within the class of propositions "x is the drawing of a white ball" must be seven to ten. Suppose, however, that the balls be replaced in the urn after each drawing, so that we have an indefinitely large number of possible drawings. Now the class of propositions can never be completely compassed, for it continues to grow larger. How can the probability of drawing a white ball be measured without some assumption? Laplace assumes that the drawings are made at random, random distribution assuring the appearance of one instance as often as any other. This amounts again to the equal distribution of ignorance.

We must escape the assumption of this doubtful canon by taking the position that probability can properly be measured, not before the fact, but only after the fact. The measure of the probability of a propositional function is a limit which the truth frequency of that propositional function as actually determined by trials tends to approach. The calculation of a probability can not be *a priori;* it must always refer back to the fact in experience; hence it must always be approximate, tentative, subject to revision. The calculations of actuaries give the best examples of the proper measurement of probabilities. They are in no sense *a priori.* Probability being thus measured after the fact and being defined as a truth frequency within a class of propositions, it is not necessary that every proposition of the class be equally possible, or—what is the same thing—equally probable. It is necessary only that the propositions of the class be distinct, numerable, and finite. It remains for experience to discover the limit which its truth frequency approaches. However, discovered or undiscovered, there must be a numerical truth frequency within a finite class of propositions. The subsequent derivation of the numerical laws of probabilities rests on this statement.

Classes of propositions, obviously, may stand in logical relations to one another. They may, for example, be related by implication, disjunction, conjunction, or opposition. Further, the assertion of such a relation between classes of propositions itself defines a new class of propositions. This third class of propositions may have a

truth frequency. Logical relations between propositional functions (we have called every propositional function a bare probability) give rise to the laws of probable reasoning just as they do to the laws of formal or necessary reasoning. Formal truth or necessity in such relations differs from probability in that the one holds for all values, the other only for a part of the values for which the relation can be asserted. Formal truth or necessity may in this way be regarded as a special case of probability.

The case of disjunction is fundamental in the logic of probabilities. The law for the addition of probabilities comes from it. Now, to assert that two classes of propositions stand formally in the disjunctive relation means: for any value of the variable, at least one of the assertions (or propositional functions) which define the two classes is true. This does not exclude their both being true. Such a disjunction would be: "Either a young man was willing to serve his country in the war or he was a traitor to it." This disjunction will, however, lose its formal character to become probable if there are values of the variable for which it is false as well as values for which it is true. It is false when both extremes of it are false, *i. e.*, when "a young man unwilling to serve his country is not a traitor to it." There being such young men, the assertion must be altered to a probable disjunction.

But how do the probabilities within each of the disjoined classes affect the probability of the disjunction itself? Since the disjunction is not exclusive, there may be cases in which both of its extremes are true, *i. e.*, in which "a young man is willing to serve his country and at the same time is a traitor to it." Only fact can determine how many such cases there are. However, each class of propositions, if it is finite, must contain a certain number of true propositions and a certain number of false. Each must have within it a numerical probability. If the disjunction be exclusive, so that it is true only when one or the other of its extremes is true exclusively, then the total number of cases of its truth must be the sum of the number of cases for which each of its extremes is true. The probability of the exclusive disjunction must, therefore, be expressible in terms of the probabilities within the two classes of propositions which it relates.

A consideration to be mentioned here and borne in mind throughout the discussion of the relations between classes of propositions is the identity of the variable. If the class of propositions defined by "x is A" be related to the class defined by "x is B" through the disjunction "x is A or x is B," it must be remembered that though x is a variable, it is the same variable throughout. The total number of values of the variable for which an assertion can be made forms one

term (the denominator) in the probability ratio for this class of propositions; the number for which the assertion is true forms the other (the numerator). Because x is the same variable throughout, for all three classes of propositions—"x is A," "x is B," and "x is A or x is B"—the denominator of the probability ratio must be the same.

From this, the identity of the variable, and from the previous definition of an exclusive disjunction, it follows that the probability of one proposition being r/x and of another s/x, the probability of their disjunction, where they are exclusive will be $(r + s)/x$. This is the usual rule for adding probabilities.

Similarly, implication between two classes of propositions may be formal or probable. The probability of an implication, however, will bear no determinate numerical relation to the separate probabilities of its two terms. But just as strict inference depends upon the implicative relation, so does probable inference. Inference proceeds from a true proposition (as hypothesis) and a true implication. Granted the truth of the implication, "If man is humble he shall inherit the kingdom of Heaven," and granted that a certain man is humble, the inference that "he shall inherit the kingdom of Heaven" is valid. All cases of an inference are numerically represented by all cases in which the antecedent of the implication upon which it proceeds is true. (Not so with all cases of the implication; for the implication may be true when the antecedent is false. A false proposition may imply any proposition.) If the implication be formally true, the inference will also be formally true; otherwise, unless formally false, the inference will be probable. Since an inference can be made only when the antecedent of the implication has a value making it a true proposition, a probable inference is, therefore, one of a class of inferences defined as all cases in which the antecedent of a probable implication is true. Thus, a class of probable inferences is defined by all cases of humility where "humility probably implies the inheritance of the kingdom of Heaven." The total number of cases of the inference (or the denominator of the ratio specifying the truth frequently in the class of inferences) will be all the cases of the truth of the antecedent; the numerator will be all cases of joint truth of antecedent and consequent, that is, all cases in which the inference is true. This fraction, again, can not be determined by any combination of the bare probabilities of the terms of the implication with one another.

The common logical consciousness makes continual and successful use of probable inference. The notion that probability has meaning only in so far as one proposition is conditioned by some other propo-

sition comes, it appears, from conceiving the probable only as that which is probably inferred, without analyzing this complex notion into the simpler one of bare probability in reference to a class of propositions. The foregoing analysis of probable inference is, moreover, essential to the definition of independence which conditions the rule for multiplying probabilities.

The multiplication of probabilities has its basis in the relation of conjunction. Two classes of propositions being formally conjoined, their defining assertions will be jointly true for all values of the variable. Being probably conjoined, they will be jointly true only for some values of the variable. As in the case of probable inference, their separate bare probabilities give no aid to the calculation of the probability of their conjunction. For no reason exists why the product of their separate probabilities should be the probability that they are jointly true. The number of cases of their joint truth is a matter wholly determined by fact. Suppose, however, they are independent in the sense that when one is true the probability that the other will be true remains unaffected. For example: It is probable that a man may be wounded in battle and it is also probable that he may receive the Croix de Guerre. The probability that he will receive the Croix de Guerre will remain the same whether or not he is wounded in battle. Hence, the inference "If he is wounded in battle he will receive the Croix de Guerre" is no more or less probable than the proposition "He will probably receive the Croix de Guerre." Their probability is equal. Symbolize the probability of the two original propositions by r/x and s/x. The probability that an inference can be drawn from one to the other must be, as we saw, the ratio of the number of cases of their joint truth to the total number of cases in which the antecedent from which the inference is drawn is true. If k represents the number of cases of their joint truth, then the fractions k/r and k/s will be the probabilities that an inference can be drawn from either one to the other of the propositions; for r and s are the total number of cases, respectively, in which the antecedents of the two possible inferences are true. (The identity of the variable, which makes the total number of cases of truth and falsity the same for both of the classes of propositions and also for their conjunction, must be again borne in mind. This, the denominator of the probability fraction for each class, is symbolized by x; and since x is assumed to be in its largest, or "factual," terms, r and s will be in their largest terms.) The probability of the joint truth of the two classes of propositions will necessarily be symbolized by k/x.

The independence spoken of above, which means that the probability "If a man is wounded in battle he will receive a Croix de

Guerre'' is no greater or no less than ''the probability that he will receive a Croix de Guerre,'' can be represented by the equation $r/x = k/s$. The probability of the inverse inference of independence will be $s/x = k/r$. These two equations, however, are exactly what result when we assume the ordinary rule for multiplying probabilities. If the product of the probabilities of the two propositions is equal to the probability of their joint truth, the equation $r/x \times s/x = k/x$ must be true. And this equation reduces to $r/x = k/s$ or $s/x = k/r$. Therefore, the arithmetical product of two probabilities will represent the probability that the two propositions will be jointly true on condition that the propositions are independent, in the sense that if one is true the probability that the other will be true will be unchanged.

It becomes evident from the discussion that, although specific probabilities like specific truths are to be measured by fact, the laws of combining probabilities into conjunctions, disjunctions, or inferences lie within the realm of pure logic; and that the laws of these fruitful methods of reasoning are intimately related to all other laws of thought.

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REVIEWS AND ABSTRACTS OF LITERATURE

Studies in Psychology. Contributed by Colleagues and Former Students of Edward Bradford Titchener. L. N. Wilson. 1917. Pp. 337.

This volume of studies was presented to Professor E. B. Titchener at a celebration of the completion of twenty-five years of distinguished service to Cornell University and to psychology. The book was edited by Professors W. B. Pillsbury, J. W. Baird, and M. F. Washburn, and contains contributions from twenty colleagues and former students. As there are nineteen separate reports, only the general character of each will be indicated.

W. B. Pillsbury discusses the principles of explanation in Psychology, testing them in the special case of the antecedents of action. J. M. Baird reports an experiment upon memory for absolute pitch. He finds it a capacity possessed in varying degrees by different individuals and present usually only under special conditions. Ferree and Rand present methods for measuring the ''Selectiveness of the Achromatic Response of the Eye to Wave-length and its Change with Change of Intensity of Light.'' J. N. Curtis tests the method of single stimulation, a rapid method of determining tactual dis-

crimination and susceptibility to visual illusion for use in anthropological and other field studies. She finds that under these special conditions of work the method is superior to others requiring a number of tests upon fewer individuals.

Problems in learning and recognition are reported by A. S. Edwards and H. M. Clarke, and problems in social psychology are discussed by M. F. Washburn and R. H. Gault. H. C. Stevens reports a modification of the Rossolimo mental tests such that their good features are retained while the time for the test is reduced from three hours to one hour. In a study of the affective tone of color combinations, L. R. Geissler derives the general law "that the greater the pleasantness of the individual constituents, the greater will be the pleasantness of the combination." C. G. Shaw discusses the psychological analysis of the religious consciousness and points out errors due to the character of consciousness and to the psychological methods used to study it.

Two studies of meaning are included in the series, one by R. M. Ogden and the other by H. P. Weld. L. D. Boring and E. G. Boring investigate the accuracy of time estimations after sleep, the nature of the designated conscious cues, and the adequacy of these cues to the temporal judgments. C. A. Ruckmich reports a study of visual rhythm. He finds in it many of the characteristics of auditory rhythm, although it is less frequent and more subject to variation among individuals. K. M. Dallenback presents an analysis of consciousness in a game of blindfold chess. Studies are reported by E. C. Sanford upon the influence of satisfaction from success and of intention to learn upon improvement. W. S. Foster contributes a bibliography of the published writings of Professor Titchener. The references are grouped under Books, Translations, Articles, Notes, Discussions (200 titles), and Editorial Work (113 titles).

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A Study of the Mental Life of the Child. H. VON HUG-HELLMUTH. Translated by James J. Putnam and Mabel Stevens. Nervous and Mental Disease Monograph Series No. 29. Washington, D. C. 1919. Pp. 154.

The monograph under consideration embodies a serious attempt to interpret the mental processes of the child, through observation of his behavior. The author, however, labors throughout under two unfortunate limitations: she begins with a mental set, which predestines all her thinking; and she does not appreciate the difference between "personal observations," and observations obtained under

carefully controlled conditions, according to the method of science.

The mental set which the author brings to her work is that of a disciple of Freud. She attributes all childish activities to a single drive—the sexual instinct. Swinging, shouting, fear of cats, general manipulation of the environment, play, laughing when others laugh, weeping when others weep, vocalization—all are of erotic origin. Does the child splash about and enjoy his bath? The excitement is sexual in character. Does the child fight and wrestle with his comrades? This behavior, too, is sexual, and portends the sexual act in adult life. Does the child become angry when thwarted, or resist his elders, or fall into a tantrum of jealousy at seeing another approved? "What else can it be than" a method of striving for erotic satisfaction? Intellectual development is also sexually motivated. "Interest in their own sex organs . . . explains, too, why boys—as a rule—acquire a greater familiarity with numbers and figures earlier than girls."

The book is full of such expressions as "I maintain that," "it seems to me," "I can confirm this from my own experience," "surely it must be," and the like. Yet there is no hint that the author regards her contribution merely as an expression of personal opinion. She generalizes extensively, and apparently is satisfied that her generalizations have the validity of scientific facts. As one reads, one's interest is diverted from the subject matter itself, and becomes absorbed in watching the influence of the point of view, as it catches every act of the child and forces it to emanate from the sexual instinct. One is tempted to try the game of showing how every childish act can be explained by reference to acquisitiveness, mastery, food-getting, or some other of the fundamental elements in the original nature of man.

The author's insistence on adequate recognition and study of the sexual instinct in children is admissible. Of course this instinct should have its share of the attention of psychological investigators, which it has, perhaps, not had in the past. One is not led to believe, however, that *all* of the attention of such investigators should be given to it. One believes merely that this author has failed to make acquaintance with Thorndike, McDougall, and William James, and that her reflections would have been illuminated by such acquaintanceship.

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COLUMBIA UNIVERSITY.

JOURNALS AND NEW BOOKS

THE AMERICAN JOURNAL OF PSYCHOLOGY. July, 1919. *Eye-Movement During Fluctuation of Attention* (pp. 241–252): H. S. LIDDELL. – Eye-movement does not cause the fluctuations in attention. No prominent eye-movements synchronize with the onset of visibility. *What is "The Unconscious"?* (pp. 253–259): HENRY JONES MULFORD. – Consciousness must be considered in terms of brain structure. "The Unconscious" then is a reflex action of the neurone and for that reason incorrectly named. *The Psychologic Aspect of Free-Association* (pp. 260–273): THEODORE SCHROEDER. – A series of free associations are presented. The author analyzes them into their relation to past experiences of a more or less erotic nature. Psychoanalytic methods are described and defended. *The Freudian Doctrine of Lapses and Its Failings* (pp. 274–290): A. A. ROBACK. – The lapses in speaking, writing and printing are explained by far-fetched, fantastic association complexes when in nearly every instance the mistake can be accounted for by associations in the immediate context material. *On Sound Discrimination in Dogs* (pp. 291–294): W. T. SHEPHERD. – Some dogs discriminate differences of musical pitch. *Confessions of an Agoraphobic Victim* (pp. 295–299): VINCENT. – The report of a man who for most of his life has had a dread of open places and broad level stretches. All bleak barren landscapes terrorized him. *Minor Studies from the Psychological Laboratory of Vassar College. Directed Egocentric Reactions* (pp. 300–302): KATHERINE B. GRAVES, EVELYN HEATH and M. F. WASHBURN. – Correlation exists between proper names and pronouns as reaction words and the tendency to personally apply stimulus words when so directed. *An Attempt to Test Moods or Temperaments of Cheerfulness and Depressions by Directed Recall of Emotionally Toned Experiences* (pp. 303–304): ELEANOR MORGAN, HELEN K. MULL and M. F. WASHBURN. – Association reactions showed a correlation with the moods as shown by the judgment of their friends. *The Healy-Fernald Picture Completion Test of the Perception of the Comic* (304–307): MARIAM A. WALKER and M. F. WASHBURN. – There is more variation among adults in the sensing of incongruous humor than among children. *The Results of Certain Standard Mental Tests as Related to the Academic Records of College Students* (pp. 307–310): HERMINE BAUM, MIRIAM LITCHFIELD and M. F. WASHBURN. – There was some correlation in academic record and test performance for the opposites, analogies and vocabulary tests. *Minor Studies from the Psychological Laboratory of William Smith and Hobart Colleges. The Comparative Sapidity of Hydrochloric, Sulphuric and Acetic Acids* (pp. 311–313): L. GIB-

SON and T. HARTMAN. – Sapidity of hydrochloric and sulphuric acids depends on their concentration in hydrogen-ions, but acetic acid presents a stronger taste than this theory will justify. *The Daylight Mazda Lamp in the Psychological Laboratory* (pp. 313–315): GILBERT J. RICH. – The lamp is dependable through the middle range of the spectrum but is deficient in blue rays. *Book Reviews.* Charles Nordman, *A Revolution in Biology and Surgery. Dead grafts.* Honorio F. Delgardo, *La Psiquiatria Psicologica:* PHYLLIS BLANCHARD. *Book Notes.* Wilfred Lay, *The Child's Unconscious Mind.* E. A. Kirkpatrick, *Studies in Psychology.* A. D. Watson, *The Twentieth Plane.* Charles Mercier, *Crime and Criminals.* J. B. Miner, *Deficiency and Delinquency.* R. F. Richardson, *The Psychology and Pedagogy of Anger.* Charles O. Whitman, *Orthogenic Evolution of Pigeons.* E. R. Squibb and Sons, *Materia Medica.* Government, *Thirty-second Annual Report of the Bureau of American Ethnology.*

East, Edwin M., and Jones, Donald F. Inbreeding and Outbreeding: their Genetic and Sociological Significance. Monographs on Experimental Biology Series. Philadelphia: J. B. Lippincott Co. 1919. Pp. 285. $2.50.

Morgan, Thomas Hunt. The Physical Basis of Heredity. Monographs on Experimental Biology Series. Philadelphia: J. B. Lippincott Co. 1919. Pp. 305. $2.50.

Walcott, Gregory Dexter. Tsing Hua Lectures on Ethics. Boston: Richard G. Badger. 1919. Pp. 193. $1.75.

Warren, Howard C. Human Psychology. Boston: Houghton Mifflin Co. 1919. Pp. xvii + 459.

NOTES AND NEWS

THE formal opening of the University of Strasbourg under French auspices took place on November 21 and 22, 1919, the anniversary of the entrance of French troops into that city after the armistice in 1918. Delegates from all the principal European universities were present at the ceremonies, as well as representatives from several American universities, including Harvard, Columbia, Michigan and Pennsylvania.

A reception for the delegates and guests was held on the evening of November 21. The formal ceremony of the opening of the university took place at 8:30 the next morning and consisted of several musical numbers and of addresses by the rector of the university, by M. Pfister, vice-president of the University Council, M. Bucher, president of the alumni association of Alsace-Lorraine and honorary president of the *cercle des jeunes étudiants,* and by President Poincaré. At one place during the exercises an opportunity was given

the delegates to come forward and deliver messages from their various universities, but the time was not long enough to permit more than a fraction to be called upon.

At the close of the programme of inauguration there was a parade past the university of French troops and members of Alsatian societies in native costumes, and immediately after this a banquet for the faculty, delegates and distinguished guests. The formalities were brought to a close by a gala programme of Massenet's "Manon" in the evening.

THE American Philosophical Association held its nineteenth annual meeting at Cornell University on December 30 and 31, 1919. The officers elected by the association for the year 1920 are as follows: President, Professor Ralph Barton Perry, of Harvard; Vice-president, Professor B. H. Bode, of the University of Illinois; Secretary-Treasurer, Professor A. H. Jones, of Brown.

THE twenty-eighth annual meeting of the American Psychological Association was held at Harvard University on December 29, 30 and 31, 1919. Six sessions were held, each dealing with some particular phase of psychology—experimental, educational, social, *etc.*—and two joint meetings with other associations, the American Association of Clinical Psychologists and the American Anthropological Association. The officers elected for 1920 are as follows: President, Professor Shepherd Ivory Franz, of St. Elizabeth's Hospital, Washington; Secretary-Treasurer, Dr. Edwin G. Boring, of Harvard; Members of the Council, Professor Herbert S. Langfeld, of Harvard, and Professor W. V. Bingham, of the Carnegie Institute of Technology.

A MEETING of the Aristotelian Society was held on December first, Professor Wildon Carr, vice-president, in the chair. Mr. G. Cator read a paper on "The Nature of Inference." The logic of the concrete universal as the medium of judgment and inference was criticized. It was shown by analysis of examples that it does not really succeed in making contact with its differences, their content is only *imputed* to it. On the other hand the instrument of inference is always an intermediating representation, particular and not universal. Absolutism, the outcome of the theory that the active dominant concrete universal is the instrument of inference, ends in the concept of reality, under the form of eternity, as an exhaustive system of differences, without character, a contentless limit. Dr. Bernard Bosanquet, in a communicated criticism, considered that Mr. Cator's view was right in so far as it rejected the linear account of inference—an affair of gaps with lesser gaps intercalated. The true general theory of inference Dr. Bosanquet described as systematic implication, or creating a partial complex in view of one's world.